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Preference Conformism: an Experiment

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Abstract

This paper reports on an experiment designed to test whether people's preferences change to become more alike. Such preference conformism would be worrying for an economics that takes individual preferences as given ('de gustibus es non disputandum'). So the test is important. But it is also difficult. People can behave alike for many reasons and the key to the design of our test, therefore, is the control of the other possible reasons for observing apparent peer effects. We find evidence of preference conformism in the aggregate and at the individual level (where there is heterogeneity). It appears also to be more consistent with Festinger's epistemic account of why it might occur than that of Social Identity Theory.

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"Sane people did what their neighbors did, so that if any lunatics were at large, one might know and avoid them."
(George Eliot, *Middlemarch*, p.7, chapter 1, iBook edition)

" ... the mind itself is bowed to the yoke; even in what people do for pleasure, conformity is the first thing thought of.... The human capacities are withered and starved."
(J.S. Mill, *On Liberty*, p.105, iBook edition)

1. Introduction

By suggesting in *Middlemarch* that people conform to the behavior of their own group so as to be able to distinguish themselves from members of other groups, George Eliot appears to anticipate Social Identity Theory (see Tajfel and Turner, 1979, and Akerlof and Kranton, 2000). In so far as such conformism arises because people's preferences become alike, her suggestion is also potentially troubling for economics. In welfare economics, for instance, it is the identification of an individual with their preferences that makes the Pareto criterion appealing. To the extent that individual preferences are endogenous and bend to those of others in some process of preference conformism, the appeal of the Pareto criterion is diminished. Likewise in both micro and macroeconomics, individual preferences are often taken as the bedrock upon which economic modeling proceeds because they are assumed not to change: 'de gustibus non est disputandum' (see Stigler and Becker, 1977, and Lucas, 1976).[1] This paper is concerned with whether there is evidence that people exhibit preference conformism.

As Social Identity Theory attests, the possibility of motivational conformism has long been important in the other social sciences. David Riesman's famous book from the 1950s, *The Lonely Crowd*, that gained him a place on the front of *Time* magazine, was based on the diagnosis that modern societies, and particularly the US,

[1] Of course, there are several models in economics where preferences are not fixed in this sense. They have typically been developed to explain behavior that appears anomalous under the standard rational choice assumptions: for example, Random Utility models, dual self models, etc.

were distinguished by the rise of a conformist attention to one's peers. In making this argument he was developing a point first made by de Tocqueville about the US. Hannah Arendt's equally, if not more, famous book from the 1950s, *The Origins of Totalitarianism*, made conformism a part of the explanation of how ordinary people were swept along by the rise of the Nazi party. Edward Said's celebrated late 20th century book, *Orientalism,* turns in part on a similar observation to that of Elliot: that individuals identify themselves through the contrast they find (or imagine) with those outside their group. Whether preference conformism occurs also matters for some evolutionary accounts of group selection. This is because, when individuals conform to the behavior of their group, the differences in individual behavior, on which selection turns, become the same as those that distinguish groups (see Boehm, 1999).

If for these reasons, it is important to know whether people exhibit preference conformism, it is also difficult. This is because people behave alike for many reasons and not just because people's preferences become alike. People often open their umbrellas when it rains - a common event triggers the similarity in behavior without any change in preferences. It also pays to do what others do when playing a coordination game. Equally people may extract information about the state of the world from the actions of others in what proves to be an information cascade yielding similarity in behavior (e.g., see Bikhchandani et al 1992). Again, even though such cascades can sometimes lead one astray, there is no doubt that it can be rational, in the sense of acting upon exogenously given preferences, to be guided by others in this way. Individuals may also act alike when they have a preference for status or social approval that comes from behaving in accordance with a social norm (see Jones, 1984, and Bernheim, 1994). A preference for following 'the fashion' can have the same effect, as can a social preference like inequality aversion in some settings (see Lahno and Serra-Garcia, 2015).

The challenge in testing for preference conformism, therefore, is to disentangle these possible 'untroubling' sources of similarities in individual behavior. This is often difficult (see Angrist, 2014) and explains why we have adopted an experimental approach. A suitably designed experiment can, in principle, control for the 'untroubling' causes of conformism. If conformist behavior is still revealed, then it points to the troubling kind of preference conformism. Our experimental design has six novel features for this purpose.

First, we focus on non-strategic decision problems to avoid the possibility that any similarity in behavior is triggered by reciprocation.[2] There are some experiments that examine whether people follow norms in social dilemmas (e.g, see Kimbrough and Vostroknutov 2016, Carpenter, 2004, Gachter et al, 2013). Such norm-following is potentially relevant because it can be interpreted as social preference conformism. However, such behavior is also consistent with a simple form of reciprocation and, to avoid this confound, we focus on non-strategic decisions where reciprocation does not naturally arise. The use of non-strategic decisions also helps avoid another possible confound: the in-group bias in social preferences that has been revealed in social dilemmas (e.g. see Chen and Li, 2009, and Hargreaves Heap and Zizzo, 2009) might also produce apparently conformist behaviours.

There are some non-strategic decision experiments that examine peer effects. Cason and Mui (1998) find that peer information has an effect in a dictator game, but this information appears to be a prime for social preferences and not for conformism as such. There are also experiments involving choices over independent lotteries,

[2] It also avoids the possibility of an apparent preference for coordination arising from subjects playing a game where coordination of behavior affects pay-offs beneficially: see Morris and Shin (2002).

where there is evidence of peer effects. That is, information about what others have chosen (which cannot convey any information about the state of the world) appears to influence individual lottery choices (e.g., see Cooper and Rege, 2011, Goeree and Yariv, 2015, Lahno and Serra-Garcia, 2015, and Gioia, 2017).[3] The peer effects are mixed in the sense that some subjects follow and some avoid what others do. Those who follow typically predominate (although see Corazzini and Greiner, 2007, where non-conformists predominate, and Duffy *et al.*, 2015, who find that, when subjects must choose private or social information and the optimal choice varies, there is a balance between the 'lone wolves', who err on the side of private, and the, 'herders', who err on the side of social information). When people follow what others do in these experiments it could be because they have a preference for sharing in misery or inequality aversion rather than because they have some tendency to preference conformism. The evidence on this is again mixed. While Corazzini and Greiner (2007) and Goeree and Yariv (2015) reject the inequality aversion interpretation, Cooper and Rege (2011) find support for the sharing in misery interpretation (what they refer to as a form of 'social regret' motivation) but Gioia (2017) rejects this possibility.[4] Our next feature is a response to this ambiguity over interpretation.

[3] There are also several experiments that examine the influence of such social information where it can contain information about the state of the world that is relevant for pay-offs: see Anderson and Holt (1997) and Weizsacker, 2010. Hung and Plott (2001) develop these information cascade experiments by considering 3 different institutional settings. One is the standard individual reward, another is a majoritarian reward and the final one is a conformist reward structure. They suggest that the fact that behaviour is different across these settings rules out preference conformism. Since the different incentives supplied by the different institutional settings clearly influence behaviour, then a simple model of preference conformism would be difficult to uphold. But this does not tell against some influence from preference conformism and the experiment highlights the difficulty of disentangling preference conformism from information transmission when both are in play.

[4] In psychology conformism experiments start with Asch's famous experiments in the 1950s. However, as the concept of preference does not play a central role in Psychology few have focused on distinguishing preference conformism from conformist behavior that arises for other reasons. The closest to our knowledge is Gino et al (2009), but this is couched in terms of the influence of norms rather than preference conformism on behavior.

Second, we have a range of different types of non-strategic decision problems. In addition to the lottery decisions discussed above, there are dictator-like distribution decisions and choices over ordinary goods. This is new and important because social regret or inequality aversion might apply to one type of problem, but it would not obviously apply to all problems. Thus if we find that individuals exhibit conformism across all types of decision problem, then, on Ockham's Razor grounds, it is more likely that that they have a general tendency towards preference conformism rather than several idiosyncratic preferences which explain their conformism across all types of decision problem.

Third, the decision problems are chosen so that the value of the options do not depend in any obvious way on a state of nature that might be revealed by other people's decisions. This avoids the possibility that behavior follows an information cascade.

Fourth, we only give individuals peer group information in the form of what someone in their group has done in the past. This avoids peer information being developed endogenously within the laboratory and so gives control. It also reduces the possibility that decision-specific preferences like social regret and inequality aversion might be triggered because the peer information refers only to 'one person'. The peer information is in this sense weak. The next design feature adds to the slightness of the group information and so contributes, with this feature, to making this a 'tough', in Popper's sense, test of preference conformism.[5]

Fifth, we assign individuals to either the 'red group' or 'blue group' (e.g. see Hargreaves Heap and Zizzo, 2009). The groups are artificial and this is likely to weaken any influence that they have on individual behavior. It also means that the

[5] 'Tough' in the sense that it increases the likelihood that the hypothesis of preference conformism will be falsified.

likelihood of subjects drawing in an habitual manner on a desire for social approval or status from behaving in accordance with a social norm that might come from membership of the same natural group outside the laboratory is much weaker. There are, for instance, no 'red group membership' habits outside the lab that might be drawn upon unconsciously inside.

Six, individual decisions remain private. This militates against the generation of any feelings of social approval or status within the lab because they depend on others knowing about your actions (although you may still derive a sense of self-image).

The two group feature of our design is also noteworthy in two respects. It allows us to test for whether any influence of peer information on behavior in the lab arises through an experimental demand effect. We explain this in more detail below. The feature also enables us to distinguish between two accounts of preference conformism in psychology. In Social Identity Theory, it may seem puzzling that individuals gain a sense of identity by acting like other members of their group, but not when there is more than one group and groups behave differently. Individuals acquire a distinct (social) identity through the differences in the way that groups behave. As a result, individuals not only pay heed to what others in their group are doing, they also take a cue on how to behave from those outside the group by avoiding in some degree what they do (see Gino et al., 2009, for experimental support and, from popular culture, Dr Seuss's 'Sneetches'). Against this view, Festinger (1954) interprets the attention to what others do differently: its origins are epistemic and come from an individual drive to evaluate one's *uncertain* opinions (and abilities). We call this 'cognitive appraisal'. It arises when a gap between one's own tentative opinion/tastes and that of others suggests a possible incorrectness or weakness in them which leads one to revise them in the direction of what others value. He argues, on the basis of a variety of experiments, that the strength of this social influence depends on the strength of a person's ties to their group. Thus what people in another group do would be less relevant than that of one's own group, but unlike Social Identity theory, their behavior would *not* be a cause for doing different.[6]

In the next section, we describe the experiment in more detail and set out the hypotheses that we test. Section 3 gives our results. We find evidence of preference conformism in the aggregate and at an individual level (and in the latter there appears to be heterogeneity). While a part of this may be the result of a demand effect, it cannot be wholly attributed to such an effect; and the evidence tends to support Festinger's account of why such conformism might occur. Section 4 discusses these results and concludes that Mill on liberty, who also worried about the prospect of conformism (see quote above), might be a better source, at least in welfare economics, for framing policy interventions once preference satisfaction loses some of its attraction.

2. Experimental design and hypotheses

Individuals came to the laboratory and were told that they would be making the same set of decisions in two stages. Instructions about the second stage were presented only after the conclusion of the first stage. There were 9 separate Decisions, as set out in Table 1. In each case there are 5 options and the individual had to choose one. The first 3 Decisions are simple choices over objects. Two of these refer to familiar objects (flowers and drinks) and one contains unfamiliar ones, the selection of a country from countries, mainly the eastern provinces of the old Soviet Union,

[6] Satayanath et al.'s, 2016, evidence on Nazi support growing fastest in cities with the densest networks of clubs and societies would seem to be consistent with the Festinger view.

ending in ‘tan’. We refer to these as the ‘label’ decisions. The next two problems are allocation, dictator-like decisions. With two commonly identified social preferences for efficiency and inequality aversion, the options in Decision 5 can be ranked (efficiency is the same but the degree of inequality changes), but the options in Decision 4 cannot be ranked in general if both considerations are in play (because there are changes in efficiency and inequality). The final 4 Decisions refer to risky options: they are the lottery decisions. The options in the last two can be ranked according to their risk.

[Table 1]

In the first stage, subjects were allocated to either a Red or a Blue group and have to choose one option in these 9 Decisions. In the second stage, subjects made the same 9 Decisions (in the same order) on five occasions (i.e., they made choices in each Decision five times consecutively). They also received information about what others had chosen. Treatments are distinguished according to this information as follows.

- Treatment 0 (Baseline) = no peer group information
- Treatment 1 = information on a person’s choice from own group in the past
- Treatment 2 = information on a person’s choice from the other group in the past
- Treatment 3 = information on a person’s choice from both groups in the past

In Treatments 1, 2 and 3, the information about own or other group choice was phrased on the screen as ‘someone in your group chose….’. We call this the own and other peer signal and we identify a conformist peer effect when the likelihood of an individual choosing an object is increased by an own peer signal of that object. Treatments 1 and 3 supply own peer signals and so provide a test for this effect.

We selected two signals (one for each ‘own’ group signal) on the basis of how people chose in each group in Treatment 0. In particular, in the Decisions that cannot be ranked (1, 2, 3, 4, 6 and 7), we selected the two options that were chosen with the smallest frequency. In the Decisions that can be ranked (5, 8 and 9) we selected the two with the biggest distance between them subject to the constraint that both were selected less than 25% of the time. In other words, we aim to have signals in the Treatments 1-3 that are rarely selected in Treatment 0 ensuring that any resulting effect of conformism should be ‘surprising’ given the individual choices. The two signals thus selected were randomly assigned: i.e. one to each group as their ‘own ‘signal (see note below Table 1 for details and online Appendix for a screenshot of how signals are presented to subjects).

Treatment 0 is also important for our test for the influence of these peer signals in Treatments 1-3 because it provides a control for any systematic changes in decision making that arise from the mere fact of repetition and which could give the appearance of a peer effect. Cooper and Rege (2011), for example, find that there is a reversion to the mean in their experiment with the result that choices naturally gravitate upon repetition towards the average of what has been observed in the past. Hypothesis 1a follows.

> **H1a (peer effects)**: After controlling for any change due to repetition, individuals tend to follow their own peer signal.

Any such tendency in the choices that we observe could arise either because all individuals have such a (possibly random) tendency to follow others or because some individuals have such a tendency. Hypothesis 1b follows.

H1b (individual consistency in peer effects): After controlling for any change due to repetition, individuals who follow their own peer signal on one or some Decision(s) are more likely to follow their own peer signal on the other Decisions.

The scope for subjects to make judgments regarding status and social approval seem more likely in Decisions where the choices can be ranked because distance between options can be meaningfully measured. Thus, in so far as we have not entirely eliminated the possible influence of a preference for status/social approval through the private nature of the decisions and the artificial creation of groups, we would expect if such preferences are in play, the peer effects will be stronger in these problems. H1c follows.

H1c (social status/approval peer effects): After controlling for any change due to repetition, individuals are more likely to follow their own peer signal in Decisions where choices can be ranked.

If a peer influence is revealed in the test of H1, then it could arise from a 'demand effect': the subjects could follow the signal regarding what others do, not because it is what others do, but in response to the fact that this is a signal from those who have designed the experiment. To test for this possibility, we compare the 'peer' effect in Treatment 1 with that in Treatment 2. There is a single signal from the experimenter in both cases and if this is all that matters, then we should expect a similar effect on behavior. If however, it is the peer group aspect of the signal that matters, then we expect the influence to be stronger when it comes from own group (Treatment1) than from the other group (Treatment 2).

H2 (demand effects only): After controlling for any change due to repetition, the peer effect is the same in Treatment 1 as in Treatment 2.

The next two hypotheses concern two views on the origins of conformist preferences and we test them using Treatment 2 and 3 where there are other group signals.

H3 (Social Identity Theory): After controlling for any change due to repetition, individuals tend to avoid the other group signal (i.e. choose it less frequently).

H4 (Cognitive appraisal): After controlling for any change due to repetition, individuals tend to follow other group signal but less frequently than their own group signal.

Experiment was conducted at the Economic Science Institute laboratory of the Chapman University. 60 subjects drawn from the general student population were randomly allocated to each one of our four treatments (Total 240 subjects). Before each stage, subjects read the corresponding instructions on their screen (see Appendix A). In each session, subjects are randomly assigned to one of four orders of the Decision sequence. These were predefined and drawn randomly at the time of the experiment (see online Appendix). Subjects were assigned randomly to either the Red/Blue group. Within each Decision, options were presented in a row and positions were randomized across periods and subjects.

Decisions 1-3 (labels) were incentivized weakly (in a gift exchange manner) with a fixed payment of 10 ECUs. In Decisions 4-5 (dictator), subjects retained the number of ECUs that they did not allocate to the other person and the ECUs they were allocated as recipient. In Decisions 6-9 (lottery), subjects received the amount of

ECUs resulting from a computerized random draw in their chosen lottery. Every choice was paid in ECUs at the end of the experiment. The exchange rate of dollars to ECUs was $1 = 75 ECUs. Subjects made an average of $15 (including $7 as show up fee) for an average of 40 minutes of experiment. This means that the pay-off from any individual choice, while non-zero, is modest. These modest pay-offs together with repetitions could encourage portfolio effects with individuals spreading choices. This would produce changes between Stage 1 and 2, but there would be no pattern of conformism in them.

3. Results

We first check for possible order, position and colour effects. Subjects faced Decisions in one of four different orders. A chi-square test rejects at the 5% level the null hypothesis that the frequency of a particular option is the same across orders in only 7 out of 45 possible cases (so, the tests cannot reject the null that frequencies are the same across the different orders in 38 out of 45 cases). Thus, there seem to be no systematic order effects. It is possible that the position of the options has an effect (e.g., subjects tend to choose the option on the far left). A chi-square test pooling data across subjects and Decisions does not reject at the 5% level the null hypothesis that the frequency of individual choices is the same across positions (p-value: 0.482).[7] Thus, there seem to be no systematic position effects. Subjects drawn from the same population are randomly allocated to either a Red or a Blue group. For each Decision in the first stage, a chi-square test rejects at the 5% the null hypothesis that individual

[7] We pool data across subjects and Decisions to address concerns of power associated to the chi-square test. For each Decision, we also fail to reject the null hypothesis that subjects' distribution of choices does not depend on the position when using Friedman tests. Every p-value is equal to 1.

choices are the same across groups in 1 out of 9 cases (Decision 6).[8] Thus, there seem to be no systematic effects associated with Red versus Blue. Finally, there should be no difference across treatments in terms of choices in the first stage. For each Decision, we compare the distributions of the four treatments and a chi-square test cannot reject the null hypothesis that choices are not different across treatments in 9 out of 9 Decisions.

We turn now to the test of our 4 hypotheses.

Table 2 gives the mean number of times (out of 5) with which subjects in different Treatments follow their 'own group signal' and 'the other group signal'. In Treatment 0 subjects are not provided with this information, we provide it here for the purposes of comparison with the other Treatments. So, for example in Decision 3, subjects in the second stage of Treatment 0 actually choose the item that we used as an own group signal in Treatment 1 less than would be expected by chance (and average 0.82 times out or 5); whereas in Treatment 1 where they receive this signal they choose the item more frequently than you would expect by chance (an average of 1.72 times out of 5). A Mann Whitney test compares the mean frequency of 'following own' and 'following other' in stage 2 of Treatments 1-3 with the control frequencies of the choice of these options in Treatment 0 (that is when choices occur for entirely personal idiosyncratic or random reasons because there is no information about what any other subject's behavior).

[Table 2]

Two things are apparent in these overall tendencies. First, there is always a tendency to follow 'own signal' in Treatment 1 (as compared with Treatment 0

[8] A similar battery of tests is done for choices in the second stage of treatment 0, where no information is provided and thus choices between Red and Blue should still come from the same distribution. Only 1 comparison out 45 is significant at 5%.

choices) and this is significant at 5% or better except for Decision 2 where it is not significant and Decisions 1 and 6 where it is significant at only 10% level. This counts in favor of H1a. The same tendency can be found in Treatment 3, but it is statistically weaker (we pick up on the fact that it is weaker when discussing Result 5). Against, H1c and the influence of status and social approval, the Decisions that can be ranked (5, 8 and 9) do not appear to have stronger peer effects than the other Decisions in either a quantitative or statistical significance sense.

These statistical tests on the aggregate data are potentially subject to a multiple hypothesis testing critique. As a result, we reproduce the analysis in Table 3 at the level of type of Decision (label, dictator and lottery). The same pattern emerges. The frequency of following 'own signal' is statistically significantly higher in Treatment 1 than the choice of the same options in Treatment 0 in each Decision type; and these differences are still statistically significant after the conservative Bonferroni adjustment of the test statistic for multiple hypothesis testing.[9]

[Table 3]

We now turn to the evidence on these points from individual random effect Poisson regressions in Table 4. In these regressions, we consider the possible influences on the count of an individual 'following own' signal and 'following other' signal for a given Decision. In each case there are several specifications. They share a control for the initial choice in stage 1 (whether it coincided with 'own signal' or 'other' signal), dummies for each Treatment and dummies for Decision types.

[Table 4]

[9] When the conservative Bonferroni adjustment to the comparison between Treatment 1 and Treatment 0 is made to the original 9 decisions, 4 remain significantly different. Alternatively a frequentist might expect 1 false positive in the 9.

First, we note, in favour of H1a that the coefficients on Treatment 1 dummy and Treatment 3 dummy (where there are own group signals) are positive and significant in the 'follow own' signal equations. The sizes of the coefficients are also interesting. It is bigger in Treatment 1 than Treatment 3, where they are respectively 70% and 50% of the size of the coefficient on initial choice. The effect of conformity in this sense may be smaller in both cases than the influence of consistency coming from 'initial choice', but, with 50-70% of the influence of consistency, these conformity effects are quantitatively non-trivial. (Again we pick up on the fact that the own group effect appears weaker in Treatment 3 than Treatment 1 below.) Second, there is weak evidence in favor of H1c as the dummy for the ranked Decisions is positive and significant at a 10% level.[10]

> **Result 1 (consistent with H1a):** Individuals are more likely to choose an object when they know a member of their own group has done so in the past.

> **Result 2 (mixed in relation to H1c):** There is only weak evidence in individual regressions and none in the aggregate data that following own signal is more likely in Decisions where the options can be ranked.

Turning to H1b, we classify a subject as a 'follower' in a Decision in Treatment 1 if they follow their own group signal when they did not initially choose this option, on at least half occasions that they encountered this Decision with the

[10] The count structure of our data (i.e. the number of times that an individual chooses a particular signal within a given Decision) leads naturally to the use of the Poisson regression. To address concerns about the appropriateness of the Poisson distribution, we check for the robustness by re-estimating the equations in Table 4 using each individual choice as the unit of observation and an OLS specification (clustered at the individual level). They are in online Appendix, Table B2. The results are qualitatively the same with this form estimation both here and in relation to the later results. One advantage of the OLS estimation is that it permits a test for time trends within a Decision in the influence of 'own' and 'other' group signals. There are no significant differences between the first and subsequent repetitions. This appears consistent with Gioia (2017). As a further robustness check, we also present in online Appendix, Table B3 the same analysis but only with the first repetition within a Decision (before any time trend is possible). The results are qualitatively the same. We thank an anonymous referee for these observations.

signal. We now use this individual Decision determination to create a ‘follower’ index for each type of Decision (Label, Dictator, Lottery): it is given by the proportion of Decisions in that type of Decision that the person is classified as a ‘follower’ (e.g. for Labels, the index takes on 0, 0.33, 0.67 or 1 as there are 3 Decisions). We now test H1b by considering whether knowing this ‘follower’ index for one type of Decision helps predict a subject’s propensity to follow the own group signal in the other types of Decision.

We do this by entering the ‘follower’ index as an explanatory variable in the regression in Table 5 on the individual frequency of following own group signal, along the lines of the earlier individual regressions in Table 4, except we now drop the observations from the Decisions that we have used to calculate the Follower index. As it helps in addressing the next hypothesis, we can do the same for the subjects in Treatment 2 to construct a ‘follower’ index of the ‘other group’ signal and we can include them in this regression and allow for a possible difference in frequency and the influence of the ‘follower’ index by a Treatment dummy that takes a value of 1 (0) for Treatment 1 (2). Table 5 gives the results (column 1 tests for the influence of the Label ‘follower’ index on the other decisions, column 2 for the influence of the Dictator ‘follower’ index, etc). The FollowerLabel, FollowerDictator and FollowerLottery by themselves are negative but not significant. The interaction with the variable treatment presents positive coefficients (and similar magnitude to the coefficient on the control for Initial choice) but it is only significant for the FollowerLottery. When we test for whether the sum of the Follower and the Follower interacted with the Treatment dummy are significantly different from zero, we find that the p-values are 0.1778, 0.0349 and 0.0545 for FollowerLabel, FollowerDictator and FollowerLottery, respectively. This means, with these definitions, that the extent to which a person is a ‘follower’ in Treatment 1 in either the dictator or lottery type of Decision in Treatment 1 helps predict (positively) their frequency of following the own group signal in the other Decisions in Treatment 1. However, it is not helpful in predicting decisions in Lottery or Dictator decisions to know the extent to which a subject is a ‘follower’ in Label decisions. In contrast in Treatment 2, none of the follower indices are themselves significantly different from zero (i.e., they only become significant when interacted with the Treatment dummy).

[Table 5]

The assumption concerning who is a follower in the construction of our Follower index is, of course, arbitrary and so these results are only illustrative. However we check for their robustness in two ways. First, we select a Decision at random for each subject and apply the same rule to classify that person as a ‘follower’ or not. In an analogous fashion, we then examined whether this classification helped predict the frequency of following the own group signal in the remaining 8 Decisions. The precise results depend on the original random selection of the Decision for the classification, but we report a typical result in the online Appendix (Table B4). The coefficient on ‘follower’ is again significant and roughly 55% of the size of the coefficient on the influence of an initial choice of this object. Second, we re-estimate equation 5 using OLS with clustered errors at the individual level. The results are stronger in terms of the predictive power of all three Follower variables in Treatment 1 Decisions and are reported in online Appendix (Table B5).

Result 3 (consistent with H1b): Knowing that a subject is a 'follower' in either dictator or lottery Decisions in Treatment 1 helps to predict their likelihood of following their own group signal in the other Decisions.

Before we turn to the remaining hypotheses, we explore tentatively what conformism looks like in our sample in Treatment 1 where the evidence of following the own group signal is strongest. First, how many conformists are there? Suppose we use our 'follower' classification in each Decision and adopt some plausible cut-off number of Decisions where being a 'follower' makes you a 'conformist'. Suppose, for instance, we define a 'conformist', for this purpose, as someone who is a 'follower' on at least 4 of the Decisions.[11] There were 23 who followed the own group signal with at least this frequency, but we exclude 7 of these subjects from being 'followers' on our definition because they also chose their own group signal initially. Our test does not allow us to distinguish whether their behavior reflects consistency or 'following'. Thus there are 16 clear, with this definition, 'conformists' in our sample of 53 where the test can distinguish (or possibly a maximum number of 23 from the sample of 60, if we put all 7 into the sample as conformists). In other words, anything between 25% and 40% our population could be 'conformist'. Of course, the definition is arbitrary. But it is not implausible and the numbers showing these 'signs' of conformism are non-trivial: they are not a small fringe even if they are in a minority.

Second, what are the characteristics of the conformists? One way of answering this is to develop an analogous definition of 'consistent' choice to identify people who show similar 'signs' but this time of 'consistent' choice: in this instance, they follow their initial choice at least twice on at least 4 Decisions. 41 of our subjects are

[11] Table B6 in the online Appendix shows how sensitive these numbers of conformists are to changes in the parameter defining follower and the parameter for the number of decisions where a subject is a follower that qualifies them for the title of 'conformist'.

‘consistent’ in this sense. These definitions of ‘conformist’ and ‘consistent’ are, of course, illustrative but they are interesting because they allow for overlap. So, people could both show signs of ‘conformism’ and ‘consistent’ choice under them. Indeed, 12 of our 16 subjects who show ‘signs’ of ‘conformism’ also show ‘signs’ of ‘consistent’ choice. In other words, most of the ‘conformists’ also shows signs of what from the point of economics looks likely perfectly normal behavior. They are not otherwise erratic choosers.[12] This is one way of thinking about whether Arendt’s conjecture that many, otherwise ordinary people are prone to conformism.

Alternatively, we could use our ‘follower’ indices as the measure of conformism and see whether individual differences in the value of these indices are related to gender or attitudes to risk and inequality. We have measures of the latter attitudes from stage 1 choices in Decisions 4, 5, 8 and 9. None of these individual controls was significant (see Table B7 in the online Appendix). In this sense, there is no obvious individual characteristic associated with conformism in our sample.

Our next result concerns the relative influence of the ‘own group’ as compared to the ‘other group’ signal. We first present the supporting evidence for the substance of this result on the relative influence and then comment on the interpretation of this evidence in relation to H2 (i.e. the hypothesis that what we have observed in Result 1 is due to a demand effect).

In the aggregate data of Table 2 and 3, there is evidence that individuals also follow the other group’s signal in Treatment 2 when this is the only signal: that is, the frequency of ‘following other’ increases between Treatment 0 and Treatment 2. However, this is a weaker effect than the one reported in support of Result 1 on the own group signal in Treatment 1. The difference in the Treatment 2 as compared with

[12] The proportion of conformists who choose consistently (12/16) is not statistically significantly different (p-value: 0.8114) from the proportion of those who are not conformists who choose consistently on these definitions (29/37).

Treatment 0 is only statistically significant in 3 Decisions (and one at 10% levels) in Table 2 and one type of Decision in Table 3. This compares with the much stronger evidence on following ‘own group’ signal in the aggregate data in Treatment 1, noted above.

The individual regressions in Table 4 also support this difference. The coefficient on Treatment 2 dummy in the ‘follow other’ signal equation is positive and significant in Table 4, but it is smaller than the analogous coefficient from Treatment 1 on the influence of the ‘own group’ signal and this difference is statistically significant (p-values: 0.0495 in (1) and 0.0192 in (2)). Further in Treatment 3 where the subjects receive both signals, the coefficient on ‘follow own’ is positive and significantly different from 0; whereas the coefficient on ‘follow other’ is not significant. Again, the ‘own group’ effect is more powerful than the ‘other group’ signal. The final piece of evidence on the difference in behavior in relation to own and other group signals comes from Table 5 (and is summarized in Result 3). Following own group signal can be useful in predicting future behavior in Treatment 1 but following the other group signal is not useful in Treatment 2.

We state that Result 4 tells against H2. This is for the following reasons. The evidence of following the other group signal in Treatment 2 is consistent with a demand effect because subjects could be responding to a piece of information provided by the experimenter. It is also consistent with the Festinger’s Cognitive appraisal hypothesis because, on this account, subjects will treat other group information as potentially indicative of what to do. Accordingly, the evidence from Treatment 2 might be thought at best to set an upper bound for the demand effect. Consequently, if the influence of the own group signal in Treatment 1 exceeds that of the other group signal in Treatment, the tendency to conformism in Treatment 1 cannot be entirely explained by a putative demand effect. We arrive at the same conclusion when we consider the evidence on individual behavior in Treatment 3 given in Table 4. Treatment 3 is the least likely to produce a demand effect because the experimenter here provides two opposing pieces of information. In this sense, there is no clear lead or prompt from the experimenter through the experimental design. If, as we find, subjects follow the own signal but not the other signal in these circumstances, it suggests that they find the own signal more salient for reasons other than mere experimental suggestion because this applies equally to the other group signal.

> **Result 4 (against H2)**: Subjects are more likely to follow 'own' signals than 'other' group signals.

Our final result concerns the direction of the influence from the other group signal. To summarise the evidence that has already been presented on this in support Result 4, it is never significant and negative. It is either positive and significant in Treatment 2 or insignificant in Treatment 3. In short, there is no evidence that subjects avoid the other group signal (as in H3) and some evidence that they are influenced in the manner suggested by H4.

The key to the interpretation of this Result, however, in relation to H3 and H4 is whether the positive effect of the other group signal in Treatment 2 is wholly a demand effect. If it is not and there is some genuine following of the other group signal, then this counts against H3 but is consistent with H4. On the other hand, if the positive effect in Treatment 2 is entirely a demand effect, then this evidence neither tells in favor nor against H3 and likewise H4 (because it is just a demand effect).

To help resolve this question, it is potentially helpful to look across all three treatments for a measure of congruence between the results of each. We have already noted that the coefficient on ‘follow other’ in Treatment 2 in Table 4 is significantly smaller than that own ‘follow own’ in Treatment 1. This difference, if we assume for this purpose that the ‘follow other’ influence arises wholly from a demand effect, captures the genuine influence of conformism. This suggests an influence of conformism in Treatment 1 equal to c.0.25 or c.20% of the effect that comes from the ‘initial choice’ control. However, in Treatment 3 where demand effects are much less likely, the size of the coefficient on ‘follow own’ is around 0.5 or 50% of the influence from the ‘initial choice’ control. This higher figure for the genuine conformist influence of the own group in Treatment 3 suggests that the ‘follow other’ coefficient in Treatment 2 cannot be wholly a consequence of a demand effect (i.e. we need to subtract something less than 0.5, the whole Treatment 2 coefficient, from the Treatment 1 coefficient to get a residual equal to 0.5 for the genuine conformist effect suggested in Treatment 3). To be specific, a demand effect equivalent to c.0.2 on the coefficient in Treatment 1 would yield a similar residual coefficient for the genuine influence of conformism in this Treatment to that found in Treatment 3 (i.e. 0.5). But if c.0.2 on the coefficient captures the demand effect, this leaves 0.3 on the Treatment 2 coefficient as genuine following of this signal.[13] For this reason, we conclude that this Result, although not decisive, mildly favours H4 over H3.[14]

[13] It is difficult to directly compare these back of the envelope calculations with the evidence on conformism in other studies because of the differences in design. Lahno and Serra-Garcia, 2015, provide the closest comparator where 33% of subjects change their choice when they know what their peers have done in one Treatment and 18% in another. Our analogous figure for those who did not choose the own signal option in stage 1 of Treatment 1 (this happened on 444 occasions) and who then chose their own signal option in the first decision of stage 2 was 105: i.e. 24% which is within the range of the Lahno and Serra-Garcia experiment. (The contrast in our experiment with the same behaviour in Treatment 0 is instructive. This occurs on 43 out 459 choices and this is statistically significantly different to the figure in Treatment 1 (p-value=0.000).)

[14] If we define someone who chooses the other signal in stage 1 of Treatment 2 (this happens on 66 occasions) and avoids this option in the first decision of Stage 2 as behaving in a manner consistent with social identity theory, then there are 26 such cases. This compares with the proportion of subjects who do the same in Treatment 0 (i.e. when they know nothing about the other signal) of 36 cases out of 73 occasions when people choose this item in stage 1. This is not statistically significant different from the proportion found in Treatment 1 (p-value=0.240). In other words, the instances of this kind of social identity consistent behaviour are no more than we find when there are no signals.

Result 5 (mildly favouring H4 over H3): Subjects do not avoid other group signal.

4. Discussion and Conclusion

In what is a 'tough' test in Popper's sense, we find evidence of a peer effect on behavior (Result 1). This evidence points to preference conformism because the other possible sources of behaving alike are unlikely to be important in these decision tasks. For example, it is difficult to be motivated by status and social approval when actions remain private; and there is only weak evidence that these peer effects are stronger in decision problems that can be ranked where the scope for such sentiments is stronger (Result 2). In addition, we find that individuals' tendencies to conformism exhibit a measure of consistency across the Decisions (Result 3). This is important. The evidence of peer effects could have arisen in our experiment because individuals had some random propensity to follow the signal of what some other person was doing. Indeed, this might be the way that an experimental demand effect would operate. However, the evidence on consistency in conformity tells against this. So does the direct evidence on demand effects: while there is some evidence that is consistent with a demand effect, it is patchy and even if taken at face value, it would not account for all the conformism we observe (Result 4).

Although this is important new evidence for the existence of preference conformism, it is perhaps not so surprising. Imitation is a well-known form of learning and in other experiments imitation captures a relatively large fraction of behaviours (e.g. see Apesteguia *et al*, 2007). Furthermore, there are evolutionary models where preferences are selected because they have survival value: i.e. preferences are, in effect, imitated and become endogenous (e.g. see Huck and

Oechssler, 1998). Where the decision tasks that people face require equilibrium selection or shared social preferences to achieve more efficient outcomes, such a process of endogenous preference formation will yield in important respects shared preferences: that is, a form of preference conformism (see Bowles and Gintis, 2011). In turn, habits of preference imitation that have evolved to solve social dilemmas may easily carry over to other types of decision problem. There are also two prominent theories in psychology that predict forms of preference conformism. Our evidence mildly seems to favour Festinger's over Social Identity theory.

Preference conformism is potentially troubling for some parts of economics and so these results are important. How troubling, of course, depends on the extent of such conformism and the nature of the trouble caused. On the numbers, our illustrative basis for classifying subjects as 'conformists' is, in this respect, arbitrary and so our figure of anything between 25% and 40% could easily change. Nevertheless, this way of coming up with a number and the fact that the coefficient in the individual regressions on the own group signal is as much as 70% of the size of the coefficient on whether someone initially revealed a preference for this object when there was no peer information suggests that the magnitude of conformist influence is non-trivial.

What, then, is the nature of the trouble? It is not deeply worrying for positive economics because modeling depends on some plausible foundational assumptions regarding behavior. What is required for this is some behavioral regularities and not that they should be derived from a model of preference satisfaction alone. In this respect the evidence from this experiment joins the growing evidence from behavioral economics that there are many regularities in behavior that are not (or not easily) derivable from the preference satisfaction model; and positive economics should take this into account. The importance of this paper in this regard is that this particular kind of non-rational choice regularity in behavior has not hitherto featured significantly in the list of behavioral economic insights.

The trouble is possibly deeper for welfare economics, however, because the appeal of the Pareto criterion depends on taking individual preferences as given. There are alternative criteria, however. Sugden (2004) is one example of an attempt to define an alternative metric of opportunity and connect this to the operation of markets when preferences are not well defined. Another comes from J.S. Mill. He is, of course, well known as an interpreter of utilitarianism and this may seem to push him in the direction of preference satisfaction, but his most famous work, *On Liberty,* presents an alternative view. In *On Liberty*, Mill wants to advance a constitution of liberty because this enables individuals to acquire their *own* character - that is, their individuality through the exercise of freedom of thought, discussion and 'experiments in living'. He did not wish here to ground policy on satisfying given preferences because people's preferences were naturally something in the making. His policy approach, like that of Buchanan (1986) later, is constitutional. Policy should be focused on establishing the rules governing outcomes and not the outcomes themselves. This, then, is the other respect in which this experiment is important. It is an encouragement to thinking more about some of the alternative to Pareto criterion approaches to welfare economics.

Nevertheless, even for constitutionalists like Mill, the evidence from this experiment is still worrying. He thought, as the quote at the beginning suggests, that conformism was the enemy of the development of individual character. One encouraging aspect of the evidence from this experiment in this respect, however, is Result 5. It seems that there is stronger evidence for the cognitive or epistemic source of conformism than the Social Identity one. This helps provide a crumb of comfort for Mill in our results. He thought that liberty would work against conformity. In so far as the exercise of liberty provides diversity, then our results support this conclusion. When there is an 'other group' as well as the 'own group' signal, the 'own group' signal effect on behavior becomes weaker.

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Table 1: Set of Decisions and options

Decision	Options				
1	SUNFLOWER	TULIP	DAISY	DAFFODIL	ROSE
2	WINE	BEER	JUICE	COFFEE	WATER
3	BHUTAN	UZBEKISTAN	KYRGYZSTAN	TAJIKISTAN	TURKMENISTAN
4	Me: 10 Other: 0	Me: 9 Other: 2	Me: 8 Other: 4	Me: 7 Other: 5	Me: 6 Other: 7
5	Me: 10 Other: 0	Me: 9 Other: 1	Me: 8 Other: 2	Me: 7 Other: 3	Me: 6 Other: 4
6	{23,1;0.5,0.5}	{16,6;0.6,0.4}	{6,26;0.7,0.3}	{10,20;0.8,0.2}	{13,3;0.9,0.1}
7	{0,20;0.5,0.5}	{4,19;0.6,0.4}	{7,17;0.7,0.3}	{9,14;0.8,0.2}	{11,1;0.9,0.1}
8	{20,0;0.5,0.5}	{18,2;0.5,0.5}	{16,4;0.5,0.5}	{14,6;0.5,0.5}	{12,8;0.5,0.5}
9	{25,0;0.6,0.4}	{23,3;0.6,0.4}	{21,6;0.6,0.4}	{19,9;0.6,0.4}	{17,12;0.6,0.4}

Note: Signal for Red group are: 1. Daisy, 2. Beer, 3. Kyrgyzstan, 4. {Me: 9; Other: 2}, 5. {Me: 9; Other: 1}, 6. {23,1;0.5,0.5}, 7. {4,19;0.6,0.4}, 8. {18,2;0.5,0.5} and 9. {25,0;0.6,0.4}; for Blue group are: 1. Daffodil, 2. Coffee, 3. Turkmenistan, 4. {Me: 6; Other: 7}, 5. {Me: 6; Other: 4}, 6. {13,3;0.9,0.1}, 7. {11,1;0.9,0.1}, 8. {14,6;0.5,0.5} and 9. {19,9;0.6,0.4}.

Table 2: Average frequency subjects follow own signal

	Treatment 0		Treatment 1	Treatment 2	Treatment 3	
Decision	own option	other option	Follow own signal	Follow other signal	Follow own signal	Follow other signal
1	0.50 (0.748)	0.57 (1.095)	1.13* (1.652)	0.63 (1.178)	0.82 (1.214)	0.65 (1.176)
2	0.57 (0.745)	0.87 (1.308)	1.40 (1.924)	0.95 (1.599)	1.72*** (1.992)	0.45*** (1.111)
3	0.82 (1.372)	0.63 (1.073)	1.70*** (1.769)	1.20 (1.735)	1.35 (1.706)	0.62 (0.958)
4	0.73 (1.260)	0.82 (1.186)	1.62*** (1.842)	1.20 (1.793)	1.30* (1.660)	0.97 (1.562)
5	0.93 (1.300)	0.80 (1.412)	1.98** (2.071)	1.27* (1.686)	1.48 (1.771)	0.83 (1.475)
6	0.93 (1.148)	0.58 (0.926)	1.57* (1.721)	1.30*** (1.453)	0.93 (1.313)	0.78 (1.277)
7	0.80 (1.038)	0.85 (1.246)	1.77*** (1.845)	1.90*** (1.811)	0.98 (1.214)	1.10 (1.526)
8	0.57 (0.981)	0.65 (0.799)	1.62*** (1.748)	1.23 (1.522)	0.95* (1.320)	0.92 (1.344)
9	0.73 (1.056)	0.66 (1.145)	1.45** (1.712)	1.07 (1.550)	1.10 (1.548)	1.07 (1.593)

Standard deviations in parenthesis. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Average frequency subjects follow own and others' signal

Decision Type	Treatment 0		Treatment 1	Treatment 2	Treatment 3	
	own option	other option	Follow own signal	Follow other signal	Follow own signal	Follow other signal
Labels	0.63 (0.653)	0.69 (0.691)	1.41*** (1.208)	0.93 (1.054)	1.29*** (1.225)	0.57 (0.592)
Dictator Games	0.83 (1.111)	0.81 (1.154)	1.80*** (1.737)	1.23 (1.533)	1.39 (1.605)	0.9 (1.340)
Lotteries	0.76 (0.605)	0.69 (0.624)	1.60*** (1.175)	1.37*** (1.135)	0.99 (0.861)	0.97 (1.015)

Standard deviations in parenthesis. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Following their own and others' signal

	Follow own signal		Follow other signal	
	(1)	(2)	(3)	(4)
Initial	1.115***	1.064***	1.267***	1.204***
	(0.048)	(0.113)	(0.056)	(0.116)
Treatment 1	0.732***	0.793***	-0.481***	-0.766***
	(0.112)	(0.120)	(0.151)	(0.167)
Treatment 2	-0.194	-0.514***	0.499***	0.536***
	(0.120)	(0.138)	(0.141)	(0.146)
Treatment 3	0.521***	0.529***	0.0456	0.0553
	(0.113)	(0.121)	(0.145)	(0.152)
Ranked	0.085	0.094*	-0.050	-0.053
	(0.053)	(0.053)	(0.059)	(0.059)
Order	0.0058	0.0070	0.00404	0.00497
	(0.009)	(0.009)	(0.010)	(0.010)
Lottery type	-0.0776	-0.0776	0.177***	0.187***
	(0.058)	(0.058)	(0.065)	(0.065)
Dictator type	-0.0157	-0.00328	0.226***	0.232***
	(0.065)	(0.065)	(0.073)	(0.073)
Initial* Treatment 1		-0.167		0.707***
		(0.136)		(0.180)
Initial*Treatment 2		0.815***		-0.146
		(0.165)		(0.152)
Initial*Treatment 3		-0.038		-0.032
		(0.146)		(0.160)
Constant	-0.621***	-0.616***	-0.927***	-0.915***
	(0.102)	(0.106)	(0.122)	(0.127)
Observations	2160	2160	2160	2160
# Subjects	240	240	240	240

Dep. variable: Number of times signal is followed within a Decision. Each Decision is one observation. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The variable 'Initial' is a control when the signal was selected in Stage 1, the dummy variable 'Treatment #' indicates treatment, the dummy variable 'Ranked' indicates if the options in the decision problem could be ranked, the variable 'Order' indicates the order of the decision problems and the variables 'Lottery type' and 'Dictator type' indicates whether the decision concerns lotteries or allocations, respectively.

Table 5: Follow signal in Treatments 1 and 2

	(1)	(2)	(3)
Initial	0.721***	0.976***	1.060***
	(0.075)	(0.071)	(0.092)
Treatment	0.042	0.014	-0.012
	(0.173)	(0.152)	(0.194)
FollowerLabel	-0.092		
	(0.624)		
Treat*FollowerLabel	0.556		
	(0.707)		
FollowerDictator		-0.292	
		(0.477)	
Treat*FollowerDictator		0.767	
		(0.531)	
FollowerLottery			-1.056
			(0.754)
Treat*FollowerLottery			1.751**
			(0.836)
Ranked Decisions	-0.045	0.071	0.155*
	(0.062)	(0.067)	(0.085)
Order	0.031***	-0.007	-0.001
	(0.012)	(0.012)	(0.013)
Female	0.181	0.123	0.108
	(0.147)	(0.132)	(0.153)
Constant	-0.109	-0.087	-0.159
	(0.164)	(0.148)	(0.174)
Observations	720	840	600

Dep. variable: Number of times signal is followed in a given task. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The variables ‘FollowerLabel’, ‘FollowerDictator’ and ‘FollowerLottery’ give the proportion of tasks in that category for which the subjects is classified as a ‘follower’.

Appendix A: Instructions

FIRST STAGE

Thanks for taking part in this experiment. You are not allowed to communicate during the experiment. If you have any question at any point, raise your hand and the experimenter will come to your desk (next to this screen).

Everyone in this room has been assigned evenly assigned into two groups: the **#group1# GROUP** and the **#group2# GROUP**. You can see the group you have been assigned to in the top left corner of the game panel.

This experiment is divided into two stages and you will see the corresponding instructions just before each stage. Now, you will see the instruction of the FIRST STAGE. In the first stage, you will be asked to make a choice in nine different tasks. These tasks have no correct answer but allow your preferences to be stated. You will be paid for each task in Experimental Currency Units (ECU). These units will be converted into dollars at the end of the experiment at the rate of $1 for every #exchange# units.

In the experiment, there are three types of tasks. These three types are explained next in this instruction screen and a corresponding sample screen will appear on the game panel.

One type is called the LOTTERY TASK. A lottery offers either of two prizes. These prizes are an amount of ECUs. One prize has certain probability (from 1% to 100%) and the other has the remaining probability to sum up to 100%. The final prize is drawn by the computer at the end of the experiment using the associated probabilities.

In this type of tasks, you will be shown 5 lotteries and you must choose one of the lotteries by clicking on it. The selected lottery will become framed in yellow. No choice will be final until you click on the submit button.

As an example, the game panel shows two lotteries. The lottery on the left offers 10 ECU with 40% probability and 3 ECU with 60% probability. The lottery on the right offers 8 ECU with 50% probability and 6 ECU with 50% probability. In the actual experiment, the position of the lotteries will be randomly assigned every single time.

To see how the lottery tasks work, select one of the two lotteries in the game panel, then click submit.

Do this now.

Another type is called the ALLOCATION TASK. In this type of tasks, you will be randomly matched with another person in this room. An allocation will indicate how to split certain amount of ECUs between you and that other person. The final allocation will be implemented at the end of the experiment.

In this type of tasks, you will be shown 5 allocations and you must choose one of the allocations by clicking on it. The selected allocation will become framed in yellow. No choice will be final until you click on the submit button.

As an example, the game panel shows two allocations. The allocation on the left offers 10 ECU for yourself, and 5 for the other person. The allocation on the right offers 8 ECUs for yourself, and 8 for the other person. In the actual experiment, the position of the allocations will be randomly assigned every single time.

To see how the allocation tasks work, select one of the two allocations in the game panel, then click submit.

Do this now.

The final type is called the LABEL TASK. A label is a just an option containing a word. The payment for each of these tasks is fixed and equal to #labelPay# ECUs. This amount will be also paid at the end of the experiment. No choice will be final until you click on the submit button.

In this type of tasks, you will be shown 5 labels and you must choose of the labels by clicking on it. The selected label will become framed in yellow.

As an example, the game panel shows two labels. The label on the left says “Left”, and the label on the right says “Right”. In the actual experiment, the position of the labels will be randomly assigned every single time.

To see how the labels tasks work, select one of the two labels in the game panel, then click submit.

Do this now.

That is the end of the instruction for FIRST STAGE. If you have any doubt so far, please raise your hand. If not, you will proceed to the first stage by clicking the start button. Once you start if, at any time, you need to review the concrete instructions of any type, they will appear at the top of the screen in the corresponding task.

Click on **Start** to being the experiment.

<u>SECOND STAGE</u>

That is the end of the FIRST STAGE. Now, you begin the SECOND STAGE of the experiment.

The payment in this second stage will be calculated as before because the same type of tasks that you have seen in the first stage will be presented now. In fact, you will see each of the same tasks that you saw in the first stage five times more.

[Treatment>0: In addition, you will also be provided specific information about the choices of other players.]

[Treatment=1: In particular, you will be informed of a choice made by members of YOUR #yourLabel# GROUP. This choice will be framed in #color# and it will be indicated by a message above the choice.]

[Treatment=2: In particular, you will be informed of a choice made by members of the OTHER #theirLabel# GROUP. This choice will be framed in #theirColor# and it will be indicated by a message above the choice.]

[Treatment=3: In particular, you will be informed of a choice made by members of YOUR #yourLabel# GROUP and the OTHER #theirLabel# GROUP. If they are different, each choice will be framed in #color# and #theirColor#, respectively. If they are the same, this choice will be framed in PURPLE. In both cases, a corresponding message above the choice will be presented.]

[Treatment>0: Recall that your own group is indicated by the top left corner of the game panel.]

Click on **Start** to continue the experiment.

Online Appendix [not intended for publication]

B1. Further details on ordering of tasks.

In order to create some variation in the order of the Decisions, we randomly drawn four orders in which the subjects would see the Decisions before the experiment. The realization of those orders is given in Table B1. For example, in version 1, subjects see Decision 3 first, Decision 7 second and so on. In each session, sets of 6 subjects were assigned randomly to one of these versions.

Table B1: Decision by Version and Order

	Version			
Order	1	2	3	4
1	3	6	5	7
2	7	1	9	4
3	2	8	3	6
4	6	2	8	9
5	4	9	7	3
6	9	4	1	5
7	8	7	6	2
8	5	3	2	1
9	1	5	4	8

Figure B1: Screenshot of a Decision

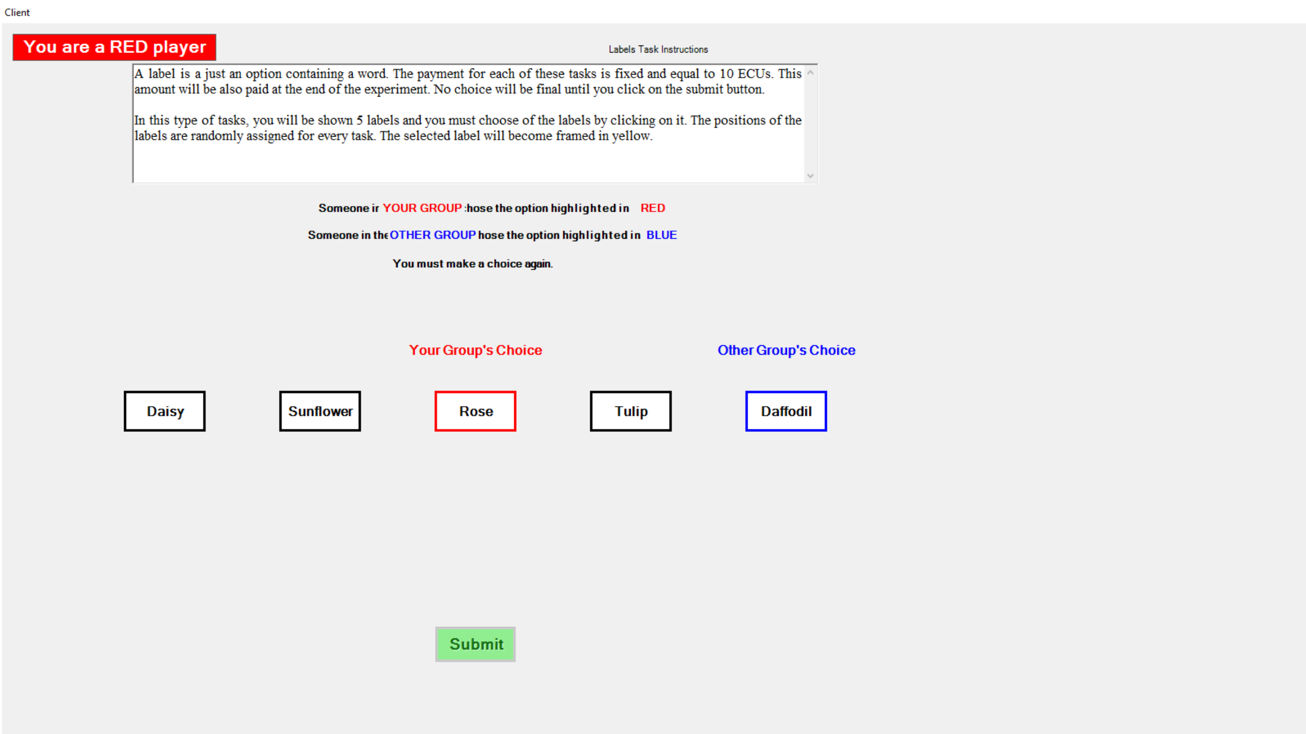

Table B2: Following their own and others' signal (OLS)

	Follow own signal		Follow other signal	
	(1)	(2)	(3)	(4)
Initial	0.365***	0.245***	0.320***	0.269***
	(0.022)	(0.040)	(0.025)	(0.049)
Treatment 1	0.160***	0.132***	-0.049***	-0.055***
	(0.023)	(0.024)	(0.014)	(0.013)
Treatment 2	-0.023*	-0.043***	0.100***	0.087***
	(0.012)	(0.012)	(0.023)	(0.023)
Treatment 3	0.100***	0.077***	0.024	0.015
	(0.020)	(0.021)	(0.020)	(0.019)
Initial*Treatment 1		0.177***		0.045
		(0.055)		(0.064)
Initial*Treatment 2		0.137**		0.099
		(0.064)		(0.076)
Initial*Treatment 3		0.163**		0.067
		(0.063)		(0.069)
Lottery Type	-0.021	-0.019	0.036***	0.035***
	(0.016)	(0.016)	(0.013)	(0.013)
Dictator Type	0.002	0.003	0.040**	0.041**
	(0.019)	(0.019)	(0.016)	(0.016)
Ranked	0.022	0.021	-0.012	-0.012
	(0.013)	(0.013)	(0.011)	(0.011)
Order	0.0006	0.0006	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Time2	0.012	0.012	-0.011	-0.011
	(0.010)	(0.010)	(0.008)	(0.008)
Time3	-0.015	-0.015	-0.0004	-0.0005
	(0.009)	(0.009)	(0.009)	(0.009)
Time4	-0.007	-0.007	0.006	0.006
	(0.009)	(0.009)	(0.008)	(0.008)
Time5	0.007	-0.006	-0.005	-0.005
	(0.009)	(0.009)	(0.008)	(0.008)
Constant	0.102***	0.119***	0.062***	0.070***
	(0.017)	(0.018)	(0.017)	(0.017)
Observations	10,800	10,800	10,800	10,800

Dep. variable: 1 if signal is followed in a given choice; 0 otherwise. The standard errors in parentheses are clustered at the individual level. * $p <$ 0.1, ** $p < 0.05$, *** $p < 0.01$

Table B3: Following their own and others' signal (OLS 1st choice)

	Follow own signal		Follow other signal	
	(1)	(2)	(3)	(4)
Initial	0.419***	0.353***	0.411***	0.413***
	(0.031)	(0.064)	(0.031)	(0.072)
Treatment 1	0.170***	0.152***	-0.053***	-0.043**
	(0.028)	(0.030)	(0.019)	(0.018)
Treatment 2	-0.011	-0.027	0.059**	0.054**
	(0.019)	(0.019)	(0.026)	(0.025)
Treatment 3	0.128***	0.122***	0.013	0.009
	(0.028)	(0.031)	(0.025)	(0.023)
Initial*Treatment 1		0.110		-0.073
		(0.088)		(0.088)
Initial*Treatment 2		0.112		0.044
		(0.082)		(0.097)
Initial*Treatment 3		0.035		0.031
		(0.098)		(0.095)
Lottery Type	-0.017	-0.018	0.043**	0.041**
	(0.024)	(0.025)	(0.020)	(0.020)
Dictator Type	0.026	0.026	0.059***	0.058***
	(0.028)	(0.028)	(0.022)	(0.022)
Ranked	0.039*	0.039*	-0.033*	-0.033*
	(0.020)	(0.020)	(0.018)	(0.018)
Order	-0.0003	-0.0003	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.074***	0.084***	0.061**	0.063***
	(0.024)	(0.024)	(0.024)	(0.023)
Observations	2,160	2,160	2,160	2,160

Dep. variable: 1 if signal is followed in a given choice; 0 otherwise. Only the first choice in each Decision is included. The standard errors are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Frequency individuals follow their own signal Treatment 1

	Follow Signal
Initial	0.893***
	(0.085)
Ranked Decision	0.067
	(0.114)
Order	0.011
	(0.015)
Lottery	0.009
	(0.105)
Dictator	0.025
	(0.152)
Follower	0.313**
	(0.147)
Constant	-0.018
	(0.138)
Observations	480
Number of subjects	60

Note: Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table B5: Follow signal in Treatments 1 and 2 (OLS)

	(1)	(2)	(3)
Initial	0.340***	0.391***	0.463***
	(0.036)	(0.0363)	(0.0517)
Treatment	-0.017	-0.0169	-0.00378
	(0.047)	(0.0339)	(0.0420)
FollowerLabel	-0.033		
	(0.100)		
Treat*FollowerLabel	0.212		
	(0.126)		
FollowerDictator		-0.0957	
		(0.0805)	
Treat*FollowerDictator		0.236**	
		(0.0942)	
FollowerLottery			-0.128
			(0.114)
Treat*FollowerLottery			0.279*
			(0.143)
Ranked Decisions	-0.008	0.0149	0.0614**
	(0.020)	(0.0232)	(0.0287)
Order	0.007*	-0.00286	-0.00286
	(0.004)	(0.00429)	(0.00416)
Female	0.045	0.0423	0.0164
	(0.035)	(0.0305)	(0.0346)
Time=2	-0.022	-0.0107	0.0117
	(0.019)	(0.0150)	(0.0137)
Time=3	0.001	0.0250	0.0267
	(0.016)	(0.0158)	(0.0161)
Time=4	0.029	0.0250	0.0300*
	(0.019)	(0.0164)	(0.0164)
Time=5	0.010	0.0321**	0.0400**
	(0.019)	(0.0153)	(0.0158)
Constant	0.161***	0.163***	0.151**
	(0.043)	(0.0367)	(0.0473)
Observations	3,600	4,200	3,000

Dep. variable: 1 if signal is followed in a given choice; 0 otherwise. The standard errors in parentheses are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FollowerLabel+ Treat*FollowerLabel=0: p-value = 0.0215
FollowerDictator+ Treat*FollowerDictator=0: p-value = 0.0044
FollowerLottery+ Treat*FollowerLottery=0: p-value = 0.0545

Table B6: Classification of subjects according to different thresholds

		Cumulative distribution of the number of tasks in which a subjects is a follower							
		7	6	5	4	3	2	1	0
Threshold to be classified as follower within a task	2	1	1	4	16	28	37	51	60
	3	0	0	2	5	12	26	40	60
	4	0	0	1	1	7	16	31	60
	5	0	0	0	1	3	10	23	60

Table B7: Predicting Follower indeces

	Follower Label	Follower Dictator	Follower Lottery
Risk averse	0.021	0.057	
	(0.023)	(0.035)	
Selfish	0.018		0.016
	(0.020)		(0.013)
Female	0.099*	-0.058	0.036
	(0.043)	(0.064)	(0.037)
Constant	0.033	0.082	0.181***
	(0.132)	(0.129)	(0.029)
Observations	120	120	120

Dep. variable: Frequency being classified as follower in each task in treatments with own signal (T1 and T3). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Further summary statistics

Results from Stage 1 by Treatment

Table B8: Distribution of choices in Treatment 0 by Decision

Decision	Options as shown in Table 1				
1	38.33	15.00	10.00	3.33	33.33
2	13.33	13.33	11.67	16.67	45.00
3	28.33	23.33	20.00	18.33	10.00
4	41.67	13.33	15.00	15.00	15.00
5	46.67	11.67	13.33	5.00	23.33
6	11.67	28.33	10.00	25.00	25.00
7	13.33	10.00	20.00	21.67	35.00
8	20.00	5.00	15.00	13.33	46.67
9	8.33	10.00	13.33	11.67	56.67

Table B9: Distribution of choices in Treatment 1 by Decision

Decision	Options as shown in Table 1				
1	16.67	20.00	16.67	8.33	38.33
2	16.67	18.33	13.33	20.00	31.67
3	16.67	41.67	13.33	15.00	13.33
4	35.00	16.67	8.33	26.67	13.33
5	35.00	8.33	11.67	15.00	30.00
6	8.33	18.33	11.67	28.33	33.33
7	5.00	10.00	20.00	30.00	35.00
8	13.33	13.33	10.00	16.67	46.67
9	10.00	10.00	8.33	8.33	63.33

Table B10: Distribution of choices in Treatment 2 by Decision

Decision	Options as shown in Table 1				
1	45.00	16.67	10.00	1.67	26.67
2	13.33	11.67	16.67	18.33	40.00
3	30.00	25.00	21.67	16.67	6.67
4	41.67	18.33	15.00	20.00	5.00
5	55.00	6.67	11.67	13.33	13.33
6	11.67	11.67	13.33	36.67	26.67
7	15.00	10.00	21.67	23.33	30.00
8	18.33	3.33	8.33	16.67	53.33
9	8.33	5.00	15.00	13.33	58.33

Table B11: Distribution of choices in Treatment 3 by Decision

Decision	Options as shown in Table 1				
1	33.33	21.67	15.00	3.33	26.67
2	16.67	23.33	21.67	11.67	26.67
3	16.67	33.33	16.67	15.00	18.33
4	53.33	11.67	5.00	21.67	8.33
5	55.00	8.33	15.00	6.67	15.00
6	5.00	20.00	16.67	40.00	18.33
7	21.67	3.33	13.33	41.67	20.00
8	18.33	8.33	6.67	16.67	50.00
9	6.67	5.00	11.67	13.33	63.33

Results from Stage 2 by Treatment

Table B12: Distribution of choices in Treatment 0 by Decision

Decision	Options as shown in Table 1				
1	37.67	18.00	12.00	9.33	23.00
2	16.00	15.00	17.00	13.67	38.33
3	26.67	25.00	15.33	19.33	13.67
4	42.67	20.00	11.67	14.67	11.00
5	47.33	14.67	10.00	8.00	20.00
6	16.67	17.00	20.33	32.33	13.67
7	22.67	14.00	23.67	20.67	19.00
8	24.00	10.00	8.33	14.33	43.33
9	13.67	7.33	15.00	14.33	49.67

Table B13: Distribution of choices in Treatment 1 by Decision

Decision	Options as shown in Table 1				
1	19.00	18.33	17.67	11.67	33.33
2	14.33	25.67	15.33	23.33	21.33
3	16.00	25.00	22.67	13.67	22.67
4	32.00	18.67	14.00	15.33	20.00
5	35.00	15.33	7.67	7.67	34.33
6	14.33	17.00	15.33	26.33	27.00
7	7.67	17.33	20.67	24.33	30.00
8	7.67	15.33	14.67	21.67	40.67
9	8.00	11.33	10.67	27.00	43.00

Table B14: Distribution of choices in Treatment 2 by Decision

Decision	Options as shown in Table 1				
1	33.67	15.00	13.33	11.00	27.00
2	13.33	13.67	15.00	21.00	37.00
3	30.00	22.67	21.33	10.67	15.33
4	39.67	21.00	10.00	15.00	14.33
5	47.33	13.67	9.33	9.67	20.00
6	12.33	19.33	15.67	30.33	22.33
7	7.67	22.33	21.67	20.33	28.00
8	19.33	12.67	13.33	23.33	31.33
9	15.33	6.67	8.33	20.67	49.00

Table B15: Distribution of choices in Treatment 3 by Decision

Decision	Options as shown in Table 1				
1	26.33	20.33	16.00	13.33	24.00
2	12.33	26.67	16.67	16.67	27.67
3	23.33	27.33	16.67	10.00	22.67
4	39.33	29.33	7.00	8.33	16.00
5	39.67	25.67	5.67	8.33	20.67
6	18.00	25.33	12.67	27.67	16.33
7	11.67	17.67	23.00	23.67	24.00
8	14.00	12.33	4.67	25.00	44.00
9	14.67	3.33	3.67	28.67	49.67

Table B16: Percentage of subjects who chose one choice in Stage 2 consistent with Stage 1 by Treatment and Decision

Decision	Treatment			
	0	1	2	3
1	5.00	16.67	5.00	6.67
2	11.67	11.67	20.00	25.00
3	16.67	10.00	13.33	15.00
4	15.00	20.00	13.33	11.67
5	18.33	25.00	11.67	16.67
6	21.67	28.33	21.67	10.00
7	25.00	23.33	18.33	6.67
8	6.67	16.67	6.67	11.67
9	15.00	8.33	13.33	6.67